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BULLETIN OF
THE NEW YORK ACADEMY
OF MEDICINE

SEPTEMBER 1944

RECENT ADVANCES IN KNOWLEDGE RELATING TO THE FORMATION, RECOGNITION AND TREATMENT OF KIDNEY CALCULI*

ALEXANDER RANDALL

Professor of Urology, University of Pennsylvania

I APPRECIATED tremendously your invitation to be present today, as well as your suggestion that the title of my remarks be, "Recent Advances in Knowledge Relating to the Formation, Recognition and Treatment of Kidney Calculi." It makes me think that perhaps I have had some influence in the elucidation of the etiology of renal stone. Unfortunately, the past few years have not allowed of further efforts in the research, and I am afraid we will have to spend a little time in reviewing the subject as studied by others, and balance their views against the theories and ideas propounded by me some years ago. As Hugh Cabot said before a group of urologists, "while cancer is our greatest problem, it belongs to all medicine, but kidney calculus is essentially a urological problem, and it belongs especially to urologists to solve."

Perhaps you will be interested in the way our work got started. Back in 1926 the American Association of Genito-Urinary Surgeons

* Presented February 25, 1944 in the Friday Afternoon Lecture series of The New York Academy of Medicine.

was meeting at Hot Springs, Va., and not being a golfer, I cornered your E. L. Keyes with some relevant questions and ideas, and it was his interested response that started the ball rolling. As the answers were not clear-cut, he suggested that we get pencil and paper and draw up specifications of what we knew and what we would like to know. Several other non-golfers joined us, and on the veranda of the hotel we gathered around a table and put down what was actually known. In so doing we found many things that were only roughly surmised, and these we proceeded to tear limb from limb. We ended with a clear estimation of what was trustworthy and what we actually did have to know and have to prove. Take, for instance, this statement by Howard A. Kelly, which is all that he could put down as fact in 1922 in regard to the etiology of renal stone formation: "The essential conditions which lead to stone formation in the kidney are imperfectly understood. Race, age, sex, habits, diet; none of them seems to play a great part." He then reports 30 cases, giving their stone characteristics, and describes each; the analysis of single, of multiple, of unilateral, of bilateral, of right-sided and of left-sided stones, and was "astonished to observe how closely his results agreed with those of other observers." Pages are devoted to age, to sex, to race, and to geographical locality, again with surprise at how closely his figures compare with others' reports, and he concludes with this statement under a heading, "Determining Factors": "The questions as to what determines the formation of stones in the kidney cannot be answered at the present time." (1922) Hinman (1935) says: "No more is known of what starts the mechanism of formation of stone than of the mechanism itself." A. J. Scholl (1936) says: "The etiology of urolithiasis is far from clear." And Hugh Young (1926): "The formation of stones in the urinary tract has long been a problem of the greatest interest and difficulty, both to pathologists and urologists. . . . while modern diagnostic methods have greatly improved the treatment. . . . yet the inability from which we still suffer, surely to prevent recurrences, makes the therapy of the disease unsatisfactory." And Keyes, in his Urology of 1917, frankly states, "The causes of stone formation are extremely obscure."

So in 1926, at the Hot Springs, we all placed our cards on the table face up, and the distressing ignorance was certainly stimulating. The question was asked, how does a stone start?; and all agreed: as a tiny crystal or a tiny cluster of crystals. But why is it not immediately

washed out of the pelvis? Perhaps some are; but some are not. Why? Where do they lodge, and why there? This constituted *problem number one*. The pelvic wall, simple and not undergoing any frequent pathologic change, was quickly ruled out; but keeping under suspicion the renal papilla. The *second problem* was the realization that stone may be composed of at least eight different salts, and the fact that they occur in urine that at one time is characteristically alkaline, and at another time highly acid, with all steps between. These facts do not help, except to exclude two often considered etiological factors, infection and stasis. *Problem number three* was the time factor, how long does a calculus take to grow to recognizable size? It is certain that a lot of them are found incidentally, and not until they start to pass, and cause ureteral blockage, do *clinically recognizable* symptoms develop. I watched one such over a period of four and one-half years, before an aeroplane trip caused dislodgement, followed by ureteral and kidney colic and, in seventy-two hours, the stone's expulsion! Diet is a frequent cause among theorists, but it falls far short of answering any of our problems, except perhaps that it plays a part in the stone's composition and which salt may predominate: this became *problem number four*. Why stone in one side at a time? There is no predominence, and while bilateral calculi are not unknown, bilaterality is rare in the majority of early cases. What causes the selection of one side, while the other remains in health, became *problem number five*.

And so we discussed and wrote down our ignorance. No one of the five, or more, problems seemed insurmountable; yet our knowledge seemed to have taken off on any one of the various tangents, and investigators seemed in each case to have spent great effort to prove a pet theory in the face of self-evident facts to the contrary. It was illuminating to see how such a group could pool the resources of medical knowledge, and how easy it was to write the specifications of what we really wanted a research to bring out and to prove.

I got off to a poor start with the idea that we wanted to create lesions and then sit back and watch stones grow, but when insurmountable difficulties arose, and stones did not grow, I awoke to perhaps the most salient fact, that it was not given to us to imitate nature and make it fit into our preconceived ideas. It was then that we chose simply to study nature and to study what occurred, and I was glad to have recourse to a large amount of autopsy material. To be sure, we

changed some of our previously held ideas, such as the prescribed methods of examining kidneys, and we opened the pelvis first, searching for pathologic states. It bore fruit. We entered the field convinced by reasoning that we should find precalculus lesions and, secondly, that we should look and expect to find such a lesion on the renal papilla. That is a true precalculus lesion. Perhaps this is the one true fact that we have uncovered, and it is my sincere regret that stress of work and present shortages of personnel have temporarily stopped our further search as to cause and perhaps prevention.

Etiology: Now it has been said, when evidence appears to be confusing, that it is wise to make simple decisions, and that wise decisions must harmonize with the fundamental truths of human nature. Certain it is that the work on the etiology of stone has produced a very confused atmosphere; some of which bears little scientific fact, while some is entirely too ultra-scientific to be of clinical value. As we approach the problem, let us first make some simple decisions. First, let us restrict ourselves to the problem of renal calculus only, omitting all reference to ureteral and vesical calculus; second, let us restrict ourselves to a study of the early, the small, the simple, uncomplicated renal calculus. This I believe a wise decision, in order to avoid the confusing evidence and the conflicting ideas when more than one pathologic state is existent. This brings us directly to the study of what is known as the "primary renal calculus," and sets aside the "secondary calculi" where other possible conditions may have etiologic bearings.

Now may I make a few statements which I believe are axiomatic statements that are again wise decisions and do not bear refutation. The first negates what I have just said, for except as it is made to apply to the clinical picture alone, there is no such thing as a primary renal calculus, there is no such thing as "calculus disease," all books to the contrary, except as a clinical entity, there is no such clinical entity until a calculus attempts to pass and causes urinary stasis and renal colic, and I would be so bold as to say that near to 10 per cent, nay, even 15 per cent, of those here present in this room have today, now, the precursors that may later cause stone to develop in a kidney. I put it this way for I am most anxious to bring to your attention a second axiomatic statement, that renal calculus is, in the final analysis, and has to be, only a symptom; a symptom that takes its origin from some precursory pathology, and it is this precursory pathology to

which we must direct our attention when we wish to seek the real etiological reason in any given case.

Two additional axiomatic statements must be made to clear our picture further: The first is that these complicated crystalline calculi, while greatly differing in their chemical structure, the one from the next, are nevertheless composed under a single chemico-physical law governing all crystallization and are, without exception, composed of salts common to the urine of all mankind, with the rare and few exceptions, cystine and xanthin, in which cases they are recognized as the common and expected urinary products in those especial patients. The second statement worthy of comment and accentuation is the time-consuming interval during which growth of a calculus is gained. An interval frequently of months and of years is required for the slow crystallization and growth of a calculus, but most important of all is the recognition that this interval is one devoid of those clinical symptoms which we are most likely to attribute to the existence of a renal calculus. We must disassociate in our long-trained minds the clinical picture that springs into existence when those two words, renal calculus, are used, and then bring forward the pathologist's impersonal viewpoint when we start to search into the early stages of calculogenesis, for these early stages are themselves as impersonal and as nonclinical as the origin, the growth and the existence of a phlebolith.

And finally, I wish to make the most positive axiomatic statement of all, one on which I feel the most emphatic, and one to which we must turn and must understand, if the science of reason and logic is to prevail, and the problem of the etiology of primary renal calculus is to be solved. This statement is that there has to be a precalculus renal lesion, an initiating lesion, a true pathologic condition existing in the kidney pelvis before the first microscopic crystal of a future calculus is laid down upon it as the necessary and essential nidus. Please grasp firmly upon the statement, a calculus has to be but the symptom of a pre-existent pathologic lesion!

So with these basic, truly axiomatic facts at hand, let us look at our problem and analyze what we can from our knowledge of today. Let us forget, for the moment, the picture in our clinically trained minds and our roentgenologically trained eyes and go back-stage, away from the actor who has been strutting these centuries before the footlights, and try to see and reason out what "makes the thing work."

That then will be the etiology.

I do not want to confuse the picture. I truly want to simplify it, even if I open myself, in the discussion, to heinous errors.

Let us look at the thoughtful and very acceptable theories that have been presented and well defended as the possible cause of renal calculus, and let us try to fit them into our need for a pre-calculus lesion and nidus:

Hyperexcretion: Probably the largest single factor that can be held responsible is the condition in which the kidney is putting out a urine overloaded with a single excretory salt. The simplest example is the hereditary cystinuric; right after him comes the patient suffering from hyperparathyroidism and eliminating hypercalcinuria; and third comes the experimental animal overfed with oxamid and excreting a hyperoxaluria. There is no essential difference between these three. The cystinuric is our perfect experimental animal, and from him we should get valuable evidence. In order to prove one of our axiomatic statements, let me ask, why do only 2.7 per cent (Hinman says 3 to 5 per cent) of the recognized cystinurics develop stone, and why does he first form a stone in only one kidney and in only one calyx of that kidney? Do we need more to make us think that there must be a reason at that point, that there must be a precalculus lesion, yes, a *precalculus* lesion? The initiating lesion! And from this I believe we are on perfectly safe ground to reason by analogy into and through the hyperparathyroid and oxamid groups, noting in passing that each group entails the same etiological and pathological sequences, but each working with a different salt. The first forms a cystine stone, the second forms a calcium phosphate stone, and the third forms an oxalate stone.

Do bear with me. There are a lot of complicating factors and unexplained observations. Let me give you an example. We had a lad of eleven years, an hereditary cystinuric, with a large single stone in his right kidney which was surgically removed and a nephrostomy drainage placed. Apparently it was a perfect differentiating state, for his nephrostomy drainage equalled his left kidney's voided urine. Dr. Andrews of the Department of Physiological Chemistry was interested, and for twelve days we continued the separate collection of each kidney's daily excretion, running as high as 480, 940 and 1050 cc. from the operated side alone. Imagine our surprise when it was observed that the unoperated left kidney continued to put out a cystinuric urine, and the oper-

ated right kidney drainage (this the kidney which had made a cystine stone) was totally free of any cystine! That the observation was perfectly balanced and repeatedly proven I need hardly say (see *J. Urol.*, 37: May, 1937, p. 655). Other excretory products were equally eliminated, but cystine was totally absent from the operated side. The fistula was allowed to heal and the boy was kept under repeated observations, and seven months later a cystoscopic differential catheterization proved that both kidneys were excreting a urine containing an equal and equivalent amount of cystine. We cannot yet explain this anomalous and confusing observation.

But these vagrancies must not be allowed to disturb our study of simple facts, for there has to be a large group of related and, at times, significant factors in our completed study. They are like the artist's gifted brush strokes that paint the background to the portrait, real, necessary, positive strokes, but we must not let them carry our attention away from the essential subject. Such is but one of the vagrancies that has helped to confuse the study in the past, and must be set aside, for the moment, in our effort to simplify the problem and cover the majority of ordinary cases.

Let us pass from the aforementioned studies, the simple, physiologic and hereditary cystinuric, the pathologic and acquired hyperparathyroidism, and the experimental, overdosed oxaluric animal; pass on to another theoretical cause and gaze on it with the same background as the foregoing; i.e., the theory of the role of hypovitaminosis A, which today includes the entire role of dietary causes. Such hypovitaminosis is accompanied by two (really three) urinary changes: (1) a consistent alkaline urine reaction, (2) a disturbance in the normal calcium-phosphorus ratio and a distinct phosphaturia, and (3), if you will, early urinary tract infection. I would like to limit this phase of our subject to the simple chemical disturbance in the urine and place these cases in the group of hyperexcretion, as seen in the cystinuric, the hyperparathyroid and the oxamid-fed animal; but other factors enter, and I wish to use them in the role in which they appear as vagrancies to the composite whole, and as factors in stone's occurrence and growth. I refer to the degenerative changes in the epithelium of the urinary tract under vitamin A deficiency, changes even to desquamation, and to the early spontaneous infection of the highly alkaline urine. Both of these coincidental factors can be recognized as contributory to crystalliza-

tion and, hence, to calculus formation. In fact, Higgins (1935) reported the interesting observation that acidification of the urine by drugs decreased the incidence of stone in animals on vitamin A deficient diets. I believe that the work on hypovitaminosis, and it is practically all animal experimentation, and devoid of human clinical studies or proof, is but another phase of an hyperexcretory state, plus local urinary tract damage, wherein such damage enters to act as the nidus about which the disturbed urine is given a chance to crystallize and form stone.

And now we are left with two further problems to elucidate and to correlate with both fact and theory in the etiology of calculus. The first is the individual who forms pure uric acid (or urate) stone, and the same in regard to the growth of pure oxalate stone; while the second problem is the role of infection.

It is difficult to make the uric acid-urate problem as plain and as simple as the previous cystine story, unless one be allowed to reason by analogy. It appears to be a complete reversal of the hyperparathyroid and the hypovitaminosis problems, as an extremely acid urine is characteristic of these uratic cases, and it is my belief that again a metabolic disturbance, probably in protein metabolism, and perhaps of liver origin, is fundamentally at fault. Certain it is that these patients who continue to pass uric acid stones (I have one who has passed 33 calculi, and another 58) constantly void a urine with a pH of 5.2 or lower, and while they frequently have attacks on first one side and then the other, I have been completely unable to demonstrate, on exhaustive studies in several such, any one thing that suggested correction; and they live happily and they form no more stones as long as the urinary pH is kept between 6.0 and 7.0.

The oxaluric also lacks all explanation as to why he so crystallizes his calculus. What we know is that it is rather characteristically single, is rarely a repeater, is extremely slow-growing and in the kidney is usually small. We know nothing that is associated with the state that bears any possible etiological significance, except that oxaluria is not a rare clinical observation, and the probable relationship to ingestive, digestive and eliminative irregularities, plus the high concentration of oxalates in certain foods.

It is my firm conviction, from the foregoing facts, that an hyperexcretory state, either periodic or constant, rules the chemical type of stone that will form, but truly plays no causative part in deciding where

or when a calculus will appear.

I have attempted to clear the picture of etiology as it may be related to four of the theories advanced, and now we come to the last; i.e., the role of infection. I do not wish to attempt an analysis of the voluminous literature that has developed, but only to point to the failure of this theory to meet the requirements necessary to a factual answer. First, let me refer to the microscopic observations on human kidneys studied in our autopsy series. In 65 of them actual calculi were present, and in 227 cases papillary calcification was observed; but in only 17 out of these 292 could we demonstrate, on microscopic study, actual evidences of infectious reactions to be associated therewith. I mean the observation of organisms, of round cell exudate and of necrosis, the picture of infectious activity as ordinarily understood and observed was absent. Second is the wide variety of organisms as observed by reporters, we having recorded 15 different organisms in 39 clinical cases. And third is the high percentage of reported sterile cases in series which the reporter himself was anxious to prove infectious, a percentage of even one-third of the cases studied. We have regularly cultured the pelvic urine at operation, not depending on catheter culture, and feeling sure, especially in obstructed cases, that with such a foreign body present the occurrence of an active infection would surely heighten and augment any bacterial process above it. We have reported an analysis of seventy-five such cases, selecting only the simple ones and omitting the evident, drastic cases of calculous pyonephrosis of long standing. In thirty-six instances, or 48 per cent, such cultures of pelvic urine were sterile, though urinary obstruction from ureteral blockage by stone was present in practically every one. A very recent case records the point at issue: a twelve-year-old child with a single stone in each side. Our studies showed a pea-sized calculus almost completely blocking the left ureter, with a rapidly developing hydronephrosis behind it; while on the right was a pelvic calculus, four times as large, but without gross obstruction. One might ask which side would be most likely to be infected? There had been no cystoscopy and no ureteral catheterization. I removed the left ureteral calculus, and released a gush of clear urine whose culture was sterile. I then removed the right renal calculus at the same operation, and found a heavy, purulent urine whose culture showed non-hemolytic Staphylococcus albus. If our theorizing in regard to the role of infection as a cause of calculus has been willfully dis-

torted by us, don't let us try to distort the truth of facts; for here, in the same patient, at the same time, one side is infected and the other, sterile, with the sterile side suffering the greater insult and damage.

To me these negative evidences speak much louder than the mere finding of an infection in the presence of a stone, especially when we know that such a stone has been relatively long resident in the kidney, and has only recently played an active part in a drastic obstructive uropathy. Perhaps I can stimulate further investigation by being disagreeable and simply saying that local infection does not, or only very rarely can, play any part in primary stone formation, and then hope that some one will promptly jump into the problem and succeed in proving, even to me, how local infection can and does cause primary calculus formation. Watch, please! I'm choosing my words, and do not want to infer "never," for again in creep those vagrancies which all medicine is trained to expect and to accept, but I am sure of my ground in the greater majority. And before dismissing the subject of infection, I want to tell you, again very bluntly, that I believe the products of infection, the toxins of distant infection, of chronic focal infection, of chronic infectious processes, and perhaps of other degenerative processes, do play a very active part in creating renal damage and papillary pathology, which assumes its position as the most important missing link in the origin and growth of a crystalline calculus.

I will show this pathology to you in lantern slides shortly, where I am sure you will be impressed by the clear-cut evidence and be better able to appreciate this simplification of an erstwhile complex problem. I hope you will perhaps even agree with me that, after all, only two essential conditions are necessary, but each necessary, in order that a renal stone may originate: first, a primary tissue damage, and second, a permanent, or transient and oft repeated, hyperexcretory state.

Treatment: There is little I can add to the subject of treatment, except to express my own handling of these cases. It has been a pleasure to have lived through the period when nephrolithotomy gradually gave way to pyelolithotomy. The elder surgeons used, almost routinely, to plunge through the renal cortex for a bloody extraction of a calculus, and left further damage with gross mattress suturing to control the active bleeding. The simple exposure of the pelvis, and, if need be, the proper enlargement of the pyelotomy into the parenchyma in cases of large stone, has much to commend it. I imagine this change has followed

the making of earlier diagnoses and, therefore, the removal of smaller calculi. This has been recently brought home to me in reviewing my teaching slides, and finding those of twenty and twenty-five years ago demonstrating conditions we rarely see today, all picturing much grosser surgical conditions. The closure of such an incision (pyelotomy) had best be left to nature, and unless unusually large, no sutures are used and no foreign material introduced, even ties, for I coagulate with electricity all bleeding points. If stitching be necessary, we use ooo and oooo plain catgut and keep it entirely outside of the urinary passageway.

In ureteral calculus two things stand out: First, the simplicity and ease of ureterolithotomy in the upper two-thirds of the ureter; and I am quite positive and adamant in my preference for surgery as against instrumental efforts in such high ureteral calculi. Second, in the lower third of the ureter the picture changes, and instrumental manipulation is the first choice, but only under quite limited and strict rules for the game. We were joking only a few weeks back about our apparent proficiency in cystoscopic removal of low ureteral stones, and it started an analysis as to why we were so successful that we were listing a stone's *removal* almost as casually as though it were the appendix. We use the Howard corkscrew and the Johnson basket, being sure that each instrument is in perfect condition, especially the latter. We demand hospitalization and operating room surroundings. But probably the most important of all is the routine use of spinal anesthesia. I want the complete anesthesia and the perfect relaxation that it produces, and I believe a fair proportion of our success is due to its consistent use. Too often I feel we mistake analgesia for anesthesia in lots of our work, and in these cases I want the complete relaxation of all muscular activity that spinal anesthesia gives. I strive for that condition delightfully expressed by Jonesco as "abdominal silence" and "postmortem relaxation."

Prevention: As we approach a better understanding of the cause of stone, we naturally look for a better ability to control its occurrence. Let me outline briefly what our knowledge allows us to demand today: First, a prompt stone analysis; and we need improvement in this problem. There is rarely a truly pure stone, and when 90 per cent of a stone is of one salt, it should be considered pure for that alone. Recent chemical testing that has been advanced is too refined, perhaps too delicate, for certainly, unless a distinctly laminated calculus is present (and I am dealing principally with the small calculi where such is rare), one

cannot expect acid and alkaline salts to be alternately deposited; yet such a report is quite within reason with a recently published technique. If uric acid is the salt, let me urge a careful metabolic study, looking for the as yet undisclosed secret, and the giving to the patient of citrocarbonate, or a similar drug, to find the dose that will raise the urinary pH to 6.0 and keep it thereabouts. The oxalate stone is likewise a difficult problem, where to me dietary regulation is a poor crutch to depend upon, and where moderate alkalinization may be tried. But I believe that in both of these salts the clearing up of all focal infections and the regular ingestion of at least 2,000 cc. of water daily are paramount. The calcium phosphate stone makers are to be studied to insure a well balanced diet, and are to be especially tested for hyperparathyroidism, and if the latter is positive, or even suggestive, exploration of the neck is urged. The taking of large quantities of water daily is perhaps the surest answer to our question of prevention, for given a dilute urine, there is little likelihood of sufficient concentration for any salt to be deposited as crystals, whether there be a nidus or not.

I believe we have a much better control today than even a decade ago, and probably a much more coöperative group of patients; but there is yet a great deal of work to be done, with, let us hope, a constant clarification of the problem and its complicated pathologic state.

Conclusions

Certain axiomatic statements can now be made in regard to calculogenesis. In this brief paper we have touched upon the five general theories of stone's formation, avitaminosis A, hyperparathyroidism, infection, colloidal imbalance and stasis, and none gives us a truly complete answer. Stone is now considered only a symptom in a very much broadened problem, and the answer is not so simple. We want, and must have, a common denominator for stone's origin, something which will act as a nidus to which the earliest crystal (be it what it may) can be attached; and for this we must go back beyond the occurrence of a visible calculus. As contributory factors we now know, not only that certain metabolic, dietary, infectious and pathologic conditions can produce the renal damage, but that, with such, also must occur the hyperexcretion of certain urinary salts, actually in supersaturation, verging on precipitation; and when these two conditions meet in the renal calyx, crystals are deposited, to grow into a primary renal calculus.

EXPERIENCE WITH ELECTRIC CONVULSIVE THERAPY IN VARIOUS TYPES OF PSYCHIATRIC PATIENTS*

LOTHAR B. KALINOWSKY

N. Y. State Psychiatric Institute

THIS presentation will deal with the clinical aspects of electric convulsive therapy. Realizing that the therapeutic possibilities of all shock methods are still limited, I shall discuss what types of psychiatric patients benefit from the treatment, and how optimal results can be obtained. The conclusions are based on various groups of psychiatric patients from two different hospitals. One series of 570 patients treated in the Psychiatric Institute, consists of voluntarily admitted patients and thus represents material seen in most research centers throughout the country. The second and larger group of almost 1200 patients from the Pilgrim State Hospital represents typical institutional material. The different results in two groups with different material treated by the same physician confirmed the view recently expressed by Dr. Lewis[1] that one of the main reasons for discordance in reports on shock therapy is the difference in material. It will be shown that an equally important reason for lack of agreement can be found in differences in the intensity of treatment.

Only a few questions of procedure will be discussed. It is still not generally understood that a convulsion is necessary for results. At the Psychiatric Institute a group of patients was treated by non-convulsive treatments alone.[2] There were no recoveries, but when the same patients, later on, were treated with generalized seizures, a fair number of them recovered. That convulsions are necessary was confirmed by several other studies. It is misleading when reports include petit mal responses in the total number of treatments given. For all practical purposes they are just as much failures as are metrazol injections without convulsive response. They can be avoided by giving a sufficiently strong current.

* Read April 11, 1944 before the Section of Neurology and Psychiatry of The New York Academy of Medicine.

We apply 100 volts for .15 seconds even with the first application. Some workers always give the highest amount of current obtainable with their machine. Animal experiments have proved the safety range of the method to be wide as far as the amount of current is concerned.[3] A useful procedure is to apply a second stimulus with higher voltage if the first stimulus did not lead to an immediate convulsion. Waiting for the chance that mere unconsciousness may yet be followed by a delayed generalized seizure makes failures unavoidable and the application must then be repeated in a patient who is already weakened. While non-convulsive responses are not infrequently accompanied by respiratory and vasomotor distress and are sometimes followed by especially long periods of confusion, application of several stimuli immediately following each other has no untoward effect; nor does it increase the strength of the convulsion which depends exclusively upon the patient's muscular development.

A sufficiently strong current also permits the therapist to be generous with sedatives where they are needed. Agitated patients should be given sedatives because psychotic exhaustion increases the danger of collapse, and continuous baths, sometimes ordered instead of sedatives, are a considerable strain on the cardiovascular system. Sedatives like hyoscin do not increase the convulsive threshold at all, but even those used as anticonvulsants have little influence on the threshold even in toxic doses, as shown in studies with bromides and barbiturates.[4] Only dilantin has a definite anticonvulsive effect. There is also no objection to the use of sodium amytal by mouth in patients who are restless after the treatment. In the more serious instances of postconvulsive excitements or furor-reactions, sodium amytal given intravenously[5] is the treatment of choice. Since these reactions are rare, we limit this procedure to those patients who in the first treatment offered difficulties.

The problem most frequently interfering with the proper and effective progress of a course of electric convulsive therapy is presented by the organic mental symptoms occurring after a certain number of convulsions. All patients show early an impairment of memory as well as emotional disturbances, but there are also more dramatic organic reactions which are usually described as confusional states, but which are sometimes taken as reactivation of the psychosis, or as shifting from one type of psychosis to another. Careful psychiatric interviews of these patients reveal that acute psychotic pictures in patients who were quiet

and unproductive prior to treatment, do not represent an aggravation of their illness but a transient organic psychotic reaction to the treatment. Korsakoff pictures, acute hallucinosis and delirium are common. Like other symptomatic psychoses they invariably clear up within one or two weeks after the last convulsion. Misinterpreted as aggravation of the patient's original psychosis, they often lead to premature discontinuation of treatment. Likewise, treatment is often unnecessarily continued because the patient still offers symptoms which actually are no longer the expression of his original illness, but organic mental changes which will disappear only after termination of treatment.

These organic reactions are certainly the most unpleasant side-effect in electric convulsive therapy. In hospitals, though, they should not interfere with the progress of the treatment; in ambulatory patients they are a serious hindrance to intense treatment.

The complications of electric convulsive therapy, as well as the questions of contraindications, were discussed in another paper from the Psychiatric Institute at the last meeting of this section.[6] I shall, therefore, limit myself to the remark that fatalities did not occur in my large material, but that I experienced instances of momentary cardiovascular and respiratory distress often enough to remain always conscious of the fact that the method is not without danger. Its indiscriminate use should be discouraged, especially by those who wish to prevent a method from falling into disrepute which proved to be valuable in various types of patients for whom no treatment was available in the past.

Conclusions regarding the possibilities and limitations of any therapeutic method are hampered by shortcomings of our psychiatric classification. Two patients with the same diagnosis can, in clinical appearance as well as in prognosis, be very different. Since it would be unjustified to delay the evaluation of a new therapeutic approach until psychiatric material is divided into better defined groups, a statistical study was limited to groups of clear-cut cases of the various psychoses. They were more easily found among the institutional material from which the figures demonstrated in this discussion are taken. Institutional material contains the greatest number of outspoken cases on the diagnosis of which psychiatrists of different schools would agree. Since many mild cases in patients who never see a psychiatrist, or are treated in clinics or open psychiatric hospitals, are automatically excluded, institutional material is not a selected group with a particularly good prognosis. It represents,

however, that group of psychotic patients which responds best to shock therapy.

The effectiveness of electric convulsive therapy is the least doubted in the manic-depressive psychosis. A depression can be cut short at any time with four to eight treatments. No greater number of treatments is necessary in deeply psychotic than in very mild cases. Here we come across a fact valid for all psychotics: The greater the loss of contact with reality the better the treatment prognosis. The figures for remissions in most reports on depressions vary between 80 and 90 per cent. In the manic phase results were discouraging as long as treatment was applied in the same way as in depressions; they are as good as in depressions if two or even three treatments are applied on the same day. This was well illustrated by a patient who had needed eighteen treatments in a severe manic excitement and who, two years later, in an equally severe manic state, cleared up under four convulsions given within 24 hours.

Electric convulsive therapy does not prevent future attacks of a manic-depressive psychosis. But patient and relatives usually consider it a great advance that we are able to cut short at will the suffering and suicidal dangers of a depression, or the implications of a manic excitement. The method fails in cyclothymic patients who continuously oscillate from one phase of the manic-depressive psychosis to the other.

In involutional melancholia results are fully as convincing as in other depressions. They are often more spectacular because, contrary to manic-depressives, involutionals can remain psychotic for years. It is interesting that several patients with psychoses of three or four years' duration did not need any more treatments than those with short depressions.

Results in the paranoid type of involutional psychosis are less favorable and not at all comparable to those obtained in pure depressions. The outcome in the paranoid group depends largely on the duration of illness, as it does in schizophrenia. Eight treatments as given in depressions, are insufficient. Now that we apply twenty, the same number as in schizophrenia, results are more satisfactory, but still do not reach half the number of remissions seen in depressions.

In view of these and other experiences in shock therapy, exact psychiatric diagnosing acquires new importance because it determines our therapeutic planning. Table I exemplifies the results obtainable in the affective disorders. The cases represented in this table were evaluated

TABLE I

RESULTS IN 200 CASES OF THE AFFECTIVE DISORDERS*

	Total	*Recovered and Much Improved*	*Improved*	*Unimproved*
Manic-depress. (depressed)	60	52 (86.6%)	4 (6.7%)	4 (6.7%)
Manic-depress. (manic)	32	27 (84.4%)	3 (9.4%)	2 (6.2%)
Involutional Melancholia	76	66 (86.9%)	8 (10.5%)	2 (2.6%)
Involutional Paranoid	32	14 (43.7%)	8 (25%)	10 (31.3%)
Total	200	159	23	18

TABLE II

RESULTS IN 275 CASES OF SCHIZOPHRENIA*

Duration	*Total*	*Recovered and Much Improved*	*Improved*	*Unimproved*
Less than 6 months	60	41 (68.3%)	13 (21.7%)	6 (10%)
6 months to 2 years	82	34 (41.5%)	24 (29.3%)	24 (29.3%)
More than 2 years	87	8 (9.2%)	36 (41.4%)	43 (49.4%)
Old cases with previous remissions	46	26 (56.5%)	10 (21.7%)	10 (21.7%)
Total	275	109	83	83

* Institutional material treated in the Pilgrim State Hospital, Brentwood, L. I. (from L. B. Kalinowsky, *Arch. Neurol. & Psychiat.*, 1943, *90*:652.)

not by the therapist but by the staff of the State Hospital. A review of the failures among the depressions showed that some had schizoid features and might belong to the schizophrenic group. Likewise unfavorable are patients with severe hypochondriacal ideas. Finally, psychoneurotic admixture accounts for a number of incomplete remissions. It is mentionable that psychotherapy was not given to these patients who only had the usual State Hospital care. This is interesting in view of the similarity between the institutional figures and those given in a follow-up of my patients at the Psychiatric Institute[7] where most patients receive at least some psychotherapy. This suggests the conclusion that psychotherapy, as desirable as it is, cannot be considered

indispensable for results with shock therapy as far as the major psychoses are concerned.

The good results in the affective disorders have overshadowed the much more limited therapeutic possibilities in schizophrenia. Yet, Cerletti and Bini devised the method for—and for a long time applied it exclusively to—the treatment of schizophrenia. When we discuss and recommend electric convulsive treatment in schizophrenia it is with the full realization that results are still limited with any of the shock methods. This, however, should not discourage their use as long as no better method is available. The psychological approach which would be available anyway only to very few patients, fails especially in those schizophrenics who respond best to shock therapy, viz., the least accessible patients who have the least contact with the outer world. The experiment offered by treating patients in the two types of hospitals showed convincingly the differences in prognosis and results of various types of schizophrenics. The group of voluntarily admitted, not acutely psychotic patients in the Psychiatric Institute, rich in borderline schizophrenics, offered poor and unconvincing results. The institutional material showed figures which were considerably higher than any comparative figures for spontaneous remissions. Institutional material permits the best comparison because it necessarily consists of patients with pronounced symptoms and a large group will have roughly the same composition in most institutions. It is neither surprising nor an objection to treatment that favorable and unfavorable prognostic factors are the same for the treatment prognosis as for spontaneous remissions. As in many other fields of medicine, we cannot expect to achieve more than to bring to a standstill an illness which is not altogether hopeless. As scientists we will have to ask for a final proof whether or not the successfully treated schizophrenics would have recovered anyway at some later time. As physicians we will apply treatment when we can hope to make the patient symptom-free at a given time. Results show that this can be done in a fair number of schizophrenics (Table II).

The most striking fact is the importance of the duration of illness. The hopelessness in cases of more than two years' duration makes early diagnosis and early treatment imperative. A prognostic factor of even greater practical importance is the type of onset, whether acute or insidious. In about 75 per cent of the patients listed as remissions, the onset was acute. On the other hand, cases of insidious onset and course, what-

ever the subtype, represent the majority of failures. A schizoid personality will not be influenced by the treatment. Therefore, extension of the concept of schizophrenia to such borderline cases would not falsify results to the better.

A follow-up of 111 of the patients paroled in the group shown in Table II revealed that within 6 months to 3 years only thirteen had to be returned, several of whom were again treated and paroled. It is felt that such figures justify treatment for the State and certainly for the individual patient and his family, although time does not yet permit to answer the question of a final cure in schizophrenics.

It is generally overlooked that Cerletti,[8] when he introduced electric convulsive therapy as a treatment for schizophrenia, gave fifteen to forty convulsions as the necessary number. Convulsive treatment is very often inadequately applied. Like insulin, it requires a long course of applications. Electric convulsive therapy in schizophrenics is successful only if twenty or more treatments are given. Discontinuation of treatment when the patient shows the first clinical improvement, leads necessarily to failure. I had lasting results in a good number of schizophrenics who, elsewhere, had been discharged after an inadequate number of treatments and relapsed after weeks. A study of figures published by Malzberg[9] for the Department of Mental Hygiene suggests the conclusion that the ratio between results in hospitals with similar material, some applying routinely twenty, others often less than ten treatments, was 5:2 in favor of the ones using intense treatment. On the other hand, the figures presented here practically repeat those given by other workers in convulsive therapy who applied twenty and more treatments. They also equal insulin figures as well as those of a recent statistical work by Braunmuehl for combined insulin and convulsive treatment.

Different indications for insulin and convulsive therapy are not yet established. Most statements made to this effect were not borne out by our experience. For instance, the statement that convulsive therapy has its best results in catatonic stupors is true only for the symptomatic breaking-up of the stupor but not regarding the final outcome which, like in insulin treatment, is less favorable in catatonic stupors than it is in paranoids. The same was recently reported by Neymann.[10] Catatonic excitements had the best outcome; paranoids follow; poorest results were seen in hebephrenics. There were no patients of any type who showed a better response when shifted from one method to the other

provided they had received adequate treatment with the first method. A systematic comparison under controlled conditions between electric convulsive therapy and insulin, both applied adequately by competent workers, is unfortunately not yet available.

One special application of electric convulsive therapy in schizophrenia should be mentioned because of its practical importance. The change in the behavior of patients after as few as three or four treatments make electric convulsive therapy especially suitable for short, purely symptomatic treatment in chronic cases where no final remissions can be obtained. A maintenance treatment of one or two weekly, bi-weekly or even monthly treatments will keep the patient on a higher level. This appears to be one of the main tasks for ambulatory electric convulsive therapy. Many patients could be kept outside the institutions if psychiatrists would limit themselves in such cases to occasional symptomatic applications.

A group to which electric convulsive therapy seems to be widely applied is represented by the psychoneuroses. Here, our own results are quite unsatisfactory. Published reports based upon small series are contradictory. The results in neurotics are comparable neither in type nor in degree to those in psychotics. The disappearance of symptoms after four convulsions, so constant in the most different psychotic syndromes, is not seen in neurotics with the exception of the so-called psychoneurotic depressions; they can be successfully treated as far as the depression is concerned, although the patient's neurotic attitude remains unchanged. Anxiety states and conversion symptoms show little lasting benefit even from a long course of electric convulsive therapy. Neurotics often show an unpleasant reaction to the memory difficulties and organic character changes which frighten some of these patients severely. Neymann,[11] therefore, warns against the use of electric convulsive treatment in neurotics. In our experience favorable responses occur occasionally mainly in the obsessive-compulsive group, and it is interesting to speculate on the relation of such cases to schizophrenia. A symptomatic use of electric convulsive therapy can be made to relieve tension states with two or three treatments in order to make patients more accessible to psychotherapy. Selinski[12] reported first on this use which appears to utilize the organic affective blurring; it often breaks down the patient's resistance in a similar way as does sodium amytal in the narcosuggestive treatment

reported here by Hoch.[13] The organic reaction of electric convulsive therapy can be used for various purposes, and it might be mentioned that I recently relieved a morphine addict from withdrawal symptoms with such short treatment. But we must realize that this is a symptomatic effect quite different from the treatment effect in psychotics. It is felt that in non-psychotics the use of electric convulsive therapy has its place only in a few carefully selected cases.

It is only for completeness sake that we mention a few experiences in epileptics. There is a definite rise of convulsive threshold after the first one or two electrically produced convulsions; this suggested attempts to give epileptics a few artificial convulsions under controlled conditions as a temporary protection against spontaneous fits. Mentioning of such attempts,[4] which succeeded in several cases, does not imply any therapeutic recommendation in a condition where anticonvulsive medication remains the treatment of choice. The only practical recommendation which can be given in the epilepsies, is the breaking up of clouded states. These usually end with one or two spontaneous convulsions. Given at will by electrical stimulation they will shorten these dangerous mental states in epileptics.

It is attempted in this paper to give a survey of work done with electric convulsive therapy. Six years of experience appear to have shown that electric convulsive therapy is a valuable therapeutic procedure. This applies mainly to those types of psychiatric patients which are the least susceptible to a psychological approach. Aside from the other prognostic signs for the various psychoses, it can be said that dramatic symptomatology and loss of contact with reality are factors favorable for a response to shock therapy; that psychoneurotic admixture in psychotics and a well preserved personality make patients usually unresponsive to shock treatment. The most striking results are achieved in the affective disorders. There is no reason for overenthusiasm in schizophrenia, but early and adequate treatment will lead to gratifying results also in schizophrenics. In all but a few types of psychoneurotics electric convulsive therapy has failed and psychotherapy remains the treatment of choice.

There is as yet much work to be done to determine scientifically the actual accomplishments of electric convulsive therapy compared to other methods of treatment. The practical results are encouraging enough to use it as a purely empirical therapeutic approach, with the

view of finding better understood and more effective methods in the future.

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THE PROBLEM OF SPECIALIZATION IN THE MEDICAL SERVICES OF THE REGULAR ARMY AND NAVY PRIOR TO THE PRESENT EMERGENCY*

LAWRENCE S. KUBIE

INTRODUCTION

THERE is a case to be made against intensive specialization in the medical services of the armed forces. The argument was admirably presented in 1923 by Capt. Carpenter† of the Navy. Capt. Carpenter reviewed the facilities for specialist training which existed at that time in both the Army and Navy medical schools, emphasizing the fact that within certain well-defined limits opportunities for specialist training were available in both. Capt. Carpenter quoted the opinions of many representatives of both services, and referred to the fact that "there are some senior medical officers of the Navy who are in favor of intensive specialization" although the majority dissented from this view. He pointed out that the scientific value of specialization is quite obvious, and that certain of its military disadvantages are equally obvious, such as the fact that the majority of military surgeons, whether Army or Navy, must be prepared to meet all emergencies in isolated posts. On the other hand there were other disadvantages, which might not occur at once to a civilian. Thus he quoted authorities who felt that for some time there had been an increasing and unwise tendency among younger medical officers to seek to specialize before they had acquired a broad base of varied experience in military medicine. He quoted others as emphasizing the danger that if there were a corps of specialists, the non-

*This study was completed nearly two years ago. At that time it was sent privately by the New York City Committee on Mental Hygiene to the Surgeons General of the Army and Navy, to certain other representatives of the medical services of our armed forces, to members of special committees of the Medical Section of the National Research Council, and to a few experienced leaders in medical administration and in medical education in this country. From these varied sources many illuminating suggestions have been received, of which full use has been made in this final version of the study. We are particularly happy to express our gratitude to Rear Admiral Ross T. McIntire (MC), Surgeon General of the U. S. Navy, and to Major General George F. Lull (MC), chief of the Personnel Service in the Office of the Surgeon General of the U. S. Army, for their helpful criticisms.

Finally we must acknowledge our special indebtedness to Mrs. P. A. Levene, for her meticulous care in compiling and checking the data on which this study is based.

† Carpenter, N., Captain, MC USN. The Military Surgeon as a Specialist, *U. S. Nav. M. Bull.* 1923, *18*:177.

specialized medical officers might come to feel that it was no longer necessary to keep up in these specialties. He raised the issue of whether it would not have an adverse effect on general morale if there were specialists who could not be sent out on routine duties, but only in groups on capital ships or to large naval installations; and whether this would not make for invidious distinctions in the matter of advancements in rank. He quoted Admiral Rossiter (then Commander and later Surgeon General) as saying, "I feel equally sure that it would be most detrimental to the efficiency of the corps should they give entire attention to these specialties, as we must have men who can go to sea as 'medical officers'; and to establish a group of shore-going or fancy-job men would be destructive of morale, as it would tend to develop a sea-going and a shore division of the corps, for which reason I believe most emphatically that they should not give their entire attention to their specialties, but must keep up with the other subjects required of a naval medical officer."

Finally Captain Carpenter emphasized the danger that young men trained as specialists at government expense might resign to go into "the more lucrative fields of civil life." In discussions of this problem with regular medical officers, one frequently encounters a tendency to by-pass the essential issue by attacking the trend toward specialization in civilian practice as being motivated largely by mercenary purposes. Thus an outstanding leader in the ranks of military medicine wrote: "It must be admitted, in the final analysis, that these things (to wit: membership in specialty societies, and certification by National Boards) are largely commercial." Although anyone who is realistic must acknowledge that economic motives play a role in civilian specialization, it is equally obvious that it is the proper use and not merely the improper abuse of specialization in medicine which must be considered in relation to the welfare of the armed services. The advantages and disadvantages of intensive specialization cannot be judged only by the behavior of those who exploit specialization for mercenary purposes.

For there is also a case to be made for the wider use of specialists in the regular medical services of the armed forces. This case was argued briefly by one whose services during the last War are remembered by all. In 1920, Col. Salmon,* wrote as follows: "I imagine that there

* Salmon, Thos. W., Col., MC USA. The Future of Psychiatry in the Army, *Mil. Surgeon*, 1920, *47*:200.

will be no dissent from the statement that the regular establishment needs to have the important specialties adequately represented at all times in its own personnel. If there were no other reason, the retention of young medical men who enjoy military life, but who would leave the service if not permitted to work along the lines of their special interests, would be sufficient, in the present shortage of medical officers. Unless special as well as general tasks in military medicine are systematically undertaken in peace, it will be useless to expect regular officers, who will occupy high administrative positions in war, to know how to use specialists to the best advantage. Hostility or at least skepticism will be their attitude if they are thrown into contact with those working in special fields with no previous association as a basis for mutual understanding and respect. It is equally important that specialists should know how to work in the army. Many a highly trained specialist from civilian life was less able to apply his skill to the actual problems of military medicine because he insisted upon conditions that could not possibly exist in a military organization."

This was the matured point of view of a specialist, who never in his civilian career had been narrowly specialized, who had exceptionally wide experience in coördinating the work of general practitioners and specialists in the problems of civilian medicine, and who saw service in our army throughout the years of the last war as chief of the psychiatric division.

It is the lesson of his experience which makes us realize the importance of discussing the problem of specialization now. Together with many other specialized activities, the psychiatric division which during the last war functioned under his leadership was discontinued soon after demobilization. Presumably this was due in part to the inevitable though short-sighted post-war cuts in congressional appropriations, and in part to the natural tendency of the Army to revert to its habitual attitude towards medical specialties as soon as the return of peace lessened the influence of civilian physicians in military medical councils. It would seem to be almost certain that unless steps are taken to forestall this, the same thing will follow the termination of this war. At present every relevant specialty is represented in the medical services of the armed forces with steadily increasing strength. As this is taking place, close working relationships are being established both between individual specialists and the "regulars," and between the Office of the Surgeon

General and the national specialty societies and certification boards. If this close coöperation is truly valuable to national defense, then it would seem wise for all concerned to take steps now to ensure its continuance after the war.

The two opposing arguments which were quoted from Captain Carpenter and Colonel Salmon make it evident that the role of the medical specialist in the armed forces presents many perplexing issues. It is by no means clear how closely it should parallel his position in civilian medicine. Before considering problems of policy, however, it is necessary to ascertain what have been both the official policies and the unofficial but traditional attitudes and practices which have determined the position of the medical specialist in the regular armed forces. In general, has specialization been encouraged or discouraged? What proportion of military physicians, if any, were customarily given an opportunity to develop an interest in any specialty? Of, these, how many were enabled to acquire complete mastery of their special fields? Or did the policies and traditions of the services limit all or most to partial and intermittent specialization? More specifically one would have to investigate the opportunities for special training, the types of medical duties, the proportion of the officer's life which he would have to devote to matters other than his special field, such as tours of general duty or administrative functions, and the original contributions in special fields which have been made by members of the medical services.*

These are questions of fact which cannot be answered fully in the midst of a war; because final answers would require a study of records in the offices of the Surgeons General of the Army and Navy, and a survey of the scientific biographies of an adequate unselected sampling of regular medical officers. Nevertheless, the questions must be asked now in the midst of war, or they may never be raised at all. Furthermore, an approximate answer to certain of these questions can be found by determining how many regular medical officers of the armed services were members of national specialty societies or were certified as diplomates of any specialty board at the end of 1938, that is before the present emergency brought increasing numbers of civilian specialists into the armed forces.

The first purpose of this communication will be to present this sta-

* For a brief sketch of the history of specialization in the Navy, see: Rossiter, P. S., Rear Adm., M.C., Surgeon General, U.S.N., Specialism and Postgraduate Training in the Medical Department of the Navy. *Mil. Surgeon*, 1936, 79:169.

tistical information. Subsequently, the concluding discussion will take up certain questions of general policy. To avoid any possible misinterpretation of the significance of the data to be presented, however, it must be stated at once that membership in national specialty societies and certification by specialty boards are only indirect indices of specialized professional proficiency. Mastery of a specialty is best shown through the type of special training, by the range and quality of clinical experience, by the amount of active teaching, and by the original contributions in the field. After the war, it will be necessary to supplement and check the data reported here by gathering such information as this from official records.

Data

These figures are compiled from information received from the secretaries of the several medical societies and medical specialty boards, checked against the information obtainable from the published transactions of the societies, the lists of names found in their special journals, the "Directory of the American Medical Association," and the 1939 "Directory of the American Board of Specialists." These figures are as correct as they can be without checking them further against questionnaires addressed to every member of each society and to every diplomate of each board. Furthermore, what errors may exist are not large enough to alter the general import of the data.

If we base our calculations on the fact that in 1938 there were about 180,000 physicians in the country, and about 1800 medical officers on active duty in the Army and Navy (or 1 per cent of the total), then military physicians might be expected to constitute something in the neighborhood of 1 per cent of the total membership of each specialty society, and 1 per cent of the diplomates of each specialty board.

Of course, such statistical expectations have only an approximate significance. Many variable factors influence the relative proportion of specialists to general practitioners in different communities, different climates, and in different occupational hazards. Such over-all percentages are only a convenient measuring rod with which to compare the representation of civilian medicine and of the medical services of the regular armed forces in the various specialties before the present emergency arose.

TABLE I

MEDICAL OFFICERS REGULAR ESTABLISHMENTS—1938

U. S. Army (active list)—1,042
U. S. Navy (active list)—815

TABLE II

MEMBERSHIP IN NATIONAL SPECIALTY SOCIETIES 1938-39

*Name of Society**	*Approx. total no. of members*	*No. of reg. Army med. officers*	*No. of reg. Navy med. officers*	*Total Army & Navy*	*Approx. statistical expectancy*
Am. Ophthalmological Soc.	185	0	0	0	2
Am. Acad. of Ophth. & Otolaryng.	2600	8	14	22	26
Am. Otological Soc.	85	0	0	0	1
Am. Laryng., Rhin. & Otol. Soc.	250	0	0	0	4
Am. Laryngological Assoc.	100	0	0	0	1
Am. Broncho-Esophagological Assn.	80	0	0	0	1
Am. Dermatological Assoc.	120	0	0	0	1
Am. Gastro-Enterological Assn.	153	0	0	0	2
Am. Proctologic Society	170	0	0	0	2
Am. Heart Assoc.	1740	8	4	12	17
Am. Assn. of Genito-Urinary Surgery	96	0	0	0	1
Am. Urological Assn.	1100	1	2	3	11
Am. Orthopedic Assn.	110	1	0	1	1
Am. Assn. for Thoracic Surgery	120	3	0	3	1
Am. Assn. of Obst., Gyn. & Abd. Surgery	150	0	0	0	2
Am. Surgical Assn.	190	1	0	1	2
The Soc. of Neurol. Surgeons	31	0	0	0	1
Am. Soc. of Anesthetists	500	0	0	0	5
Assn. of Am. Physicians	210	1	0	1	2
Nat. Tuberculosis Assn.	1200	1	0	1	12
Am. Society of Tropical Med.	400	14	8	22	4
Am. Therapeutic Soc.	150	1	0	1	2
Am. Soc. for Clinical Invest.	325	0	0	0	3
Am. Soc. for Pharm. & Exper. Ther.	203	0	0	0	2
Am. Physiological Soc.	660	0	0	0	7
Am. Soc. for Exp. Pathology	226	0	0	0	2
Am. Soc. of Clinical Pathologists	650	0	0	0	7
Soc. of Am. Bacteriologists	1150	1	0	1	12
Am. Assn. of Path. & Bacteriologists	300	7	0	7	3
Am. Neurol. Assn.	259	0	0	0	3
Am. Psychiatric Assn.	1762	3	1	4	18
Am. Psychoanalytic Assn.	175	0	0	0	2
Am. Public Health Assn.	2200	17	5	22	22
Am. Clinical & Clinatological Assn.	175	0	0	0	2
Am. Congress of Physical Therapy	?	0	0	0	?
Am. College of Physicians	4200	134	83	217	42
Am. College of Surgeons	11500	273	220	493	115

*None of the purely military medical societies is included in this tabulation, for reasons that will become clear in the discussion (vide infra).

TABLE III

DIPLOMATES OF SPECIAL BOARDS—1939

	Total (approx.) no. of diplomates	*No. of reg. Army med. officers*	*No. of reg. Navy med. officers*	*Total Army & Navy*	*Approx. statistical expectancy*
Am. Board of Anesthesiology	83	0	0	0	1
Am. Board of Dermatol. & Syphilology	475	0	0	0	5
Am. Board of Internal Medicine	2050	35	12	47	21
Am. Board of Ophthalmology	1300	10	6	16	13
Am. Board of Orthopaedic Surgery	660	0	0	0	7
Am. Board of Otolaryngology	2770	21	14	35	28
Am. Board of Plastic Surgery*					
Am. Board of Psychiatry & Neurol.**	700	2	2	4	7
Am. Board of Radiology***	1280	5	7	12	12
Am. Board of Surgery	1210	4	4	8	12
Am. Board of Urology	590	0	0	0	6
Am. Board of Neurosurgery	(not then organized)			?	?

*List of diplomates omitted from 1939 edition of Directory at the request of Board.
**103 additional candidates not in above figures, because biographical data was received too late to permit inclusion in that issue of the Directory.
***74 additional candidates, for the above reason.

SUMMARY OF THE SITUATION IN 1938-39

1. Of the thirty-seven societies listed, the Army and Navy were not represented at all in twenty-one.

2. In the remaining sixteen, the Army alone without the Navy was represented in seven.

3. Furthermore, in the remaining sixteen, the total representation from the armed forces was less than the approximate statistical expectancy in eight.

4. Of the eight societies in which the services were represented up to the statistical expectancy, two were not specialty societies (The American Colleges of Physicians and of Surgeons); and two were in fields of special interest to the armed forces (Public Health and Tropical Medicine), and therefore do not furnish an indication of military representation in specialties in general.

5. This leaves statistically expected representation relevant to the purposes of this study in only four specialty societies: i. e., the American Academy of Ophthalmology and Otolaryngology; the American Orthopaedic Association; the American Association for Thoracic Surgery; and the American Association of Pathology and Bacteriology.

6. In analyzing the figures by specialties instead of by societies, one finds that as of 1938-1939 the Army and Navy were represented in one out of six societies in the field of ophthalmology and otolaryngology,

with no representation in dermatology, gastroenterology, proctology, neurosurgery, anesthesiology, pharmacology and experimental therapeutics, physiology, experimental pathology, neurology, climatology, or physical therapy. Of the special subdivisions of surgery, there was statistically expected representation in orthopedic and thoracic surgery only, and in these from the Army alone.

If one totals the data on membership in national specialty societies, omitting the four special cases referred to under paragraph 4, page 501, one finds that the statistical expectancy of representation from the armed services is 156. The actual total membership of regular medical officers of the Army and Navy is 59. Or, to put it another way, the armed services in 1939 were represented in national specialty societies about one-third as frequently in relation to their total numbers as were civilian physicians.*

7. Among the diplomates of specialty boards, one finds no representation from the armed forces in five (anesthesiology, dermatology, syphilology, orthopedic surgery, and urology); less than the statistically expected representation in psychiatry and surgery; and statistically expected representation in internal medicine, ophthalmology, otolaryngology, and radiology.

8. In 1939, approximately 8 per cent of the physicians of the country were diplomates of some board. In the same year about 6 per cent of all regular Army and Navy physicians were certified. For the country as a whole, approximately 70 per cent of all diplomates were certified in some specialty *other than general medicine or surgery*. Among the diplomates from the regular armed services, however, about 54 per cent were certified in some specialty other than general medicine or surgery, and of this 54 per cent, three-quarters were certified in ophthalmology or otolaryngology. This concentration of diplomates in these two fields is perplexing.

Discussion

As has already been pointed out, membership in national specialty organizations is only one of several ways in which a man may manifest

*The question should be asked whether our national specialty societies have been fully alive to their responsibilities—whether through the years of peace they have sought to maintain a close liaison with the offices of the Surgeons General, and whether they have impressed on their memberships the importance of being adequately represented in the ranks of military medicine, in order that they should be prepared to make their special contributions to our national strength. Certainly the Specialists' Reserve Corps, the development of which was undertaken by the Navy, should be encouraged and supported by active campaigns in every national specialty society.

his proficiency in a specialty. Nor is it the best indication. Moreover, membership in a few of the national societies is based upon membership in local sections, to which military medical officers may not be eligible because of their relatively nomadic existence. (The American Urological Association has recently altered its rules so as to exempt military medical officers from this requirement.) Again some national societies require that the physician must practice the particular specialty alone; which in turn may automatically exclude otherwise qualified military medical officers.

In the course of time, certification will probably become a better indication of specialized competence, but in 1938 the principle of certification was still young; and even now not every qualified physician, whether civilian or military, submits to these examinations.

We are left therefore, with facts which are only partially informative, but which indicate that further investigation of the problem is desirable. In the meantime it is reasonable to ask how it happens that among the specialties (as opposed to general medicine or surgery, and excluding public health and tropical medicine as special cases), regular medical officers from the armed forces were represented as fully as their civilian colleagues in national specialty societies and among the diplomates of specialty boards only in the fields of ophthalmology, otolaryngology, and to a lesser extent in thoracic and orthopedic surgery.

Furthermore, since we are seeking evidence of specialization, it is important to emphasize the fact that membership in the American College of Physicians or in the American College of Surgeons, or certification by the American Board of Internal Medicine, while important as indication of a high level of general professional excellence, is not an indication of specialization. Indeed it might be argued that it might be well to require that every physician in the armed forces who is not trained as a specialist in some particular field should qualify as a member of the American College of Physicians or Surgeons; and that any such physician should be certified either by the American Board of Internal Medicine or by the American Board of Surgery, unless he is certified by some other specialty board.

The reason for this is presented in a letter from the Surgeon General of the Navy. He points out that in war as in peace, the physician in the armed services functions under more varied circumstances than those which confront the civilian practitioner. Admiral McIntire goes on to

say that the naval medical officer "must be qualified properly to treat any type of medical or surgical emergency which may arise when he is the one physician in a ship at sea; must be qualified to find the proper solutions for problems of hygiene, sanitation, and public health, arising in such an isolated community, which may within a short time pass from one extreme of weather to another; and must also be qualified as a medical administrator." (Personal communication December 5, 1942). These words describe with precision the challenge which confronts a large proportion of military physicians in the Army, the Navy, and the Air Forces. They indicate that the medical officer in such posts must be both administrator and general practitioner of exceptional ability.

No one can disagree with this description of the duties of the medical officer on isolated posts. But in the same communication the Surgeon General makes it clear that *every* medical officer is held to these requirements: i.e., although "Naval medical officers are encouraged to develop aptitude and experience in the various specialist fields. . . . the very nature of their basic specialty, however, (i.e., naval medicine) does not permit their concentrating upon one limited field." Similarly, Major General Lull writes: "When a young doctor enters the Medical Corps of the Army or Navy, there are certain things that he should make up his mind that he must sacrifice if he is to be successful. One of these is any high degree of medical specialization. He must become a specialist in military medicine instead. . . ." (Personal com., Oct. 30, 1942).

Thus both services operate on the assumption that every medical officer will meet the same challenges, must be ready to perform the same tasks, and must therefore go through the same mill. Every medical officer without exception must become a Jack of all the medical trades; yet somehow this is to be achieved without paying the penalty usually paid by the proverbial Jack of the adage. Somehow he must become in some degree a master of all; and the problem becomes the question of how this is to be achieved. Who is to teach him? Who is to establish practical standards of achievable proficiency in the many special fields he is supposed to master? And what happens to the man who may have exceptional aptitude in one field but who lacks the diffuse facility which makes possible the mastery of many? Has he no contribution to make in peace or in war to military medical science?

The entire problem is not unlike that which confronts the medical missionary in an isolated post; and if the military physician is to meet

those demands well it would seem that he must be trained as the best medical missionary is trained, through a series of internships in one special field after another, with interspersed periods of duty in which to test and perfect his mastery of each newly acquired special technique. In the end he will emerge from such a series of apprenticeships not a specialist in any one field but a superb general practitioner, equipped with a mastery of more of the special techniques of medicine and surgery than is possessed by the general practitioners of civilian medicine. Rarely, he may become an investigator; but more often he will be an applied medical scientist in the best sense of the word. One can only wish that the general practitioners of civilian medicine were held to the same high standards of broad and inclusive proficiency.

It is indeed clear that the military "general practitioner" and administrator is, as Admiral McIntire calls him, a "Specialist in Military Medicine" (loc. cit.), and that he will always be the backbone of military medicine. Nevertheless, it is necessary to consider whether the efficiency of this "Specialist in Military Medicine" and the efficiency and standards of the service as a whole could be raised to still higher levels by making more of a place in the medical services of the regular armed forces for the specialist as we know him in civilian medicine.

For instance, the succession of specialized apprenticeship, which has been described as the ideal program of postgraduate medical instruction for military needs, and which Admiral McIntire also refers to in his letter, requires the services of specialists; because in any field of medicine it is only the specialists whose teaching can keep pace with the advancing frontiers of knowledge.

Again, as Major General Lull points out (loc. cit.), in medical as in combatant troops the regular personnel constitutes the nucleus about which the organization must grow when the armed forces expand rapidly in an emergency. For this reason, among others, it would seem to be important to have the specialties fully represented in the ranks of medical "regulars", forming a cabinet of specialists around the Surgeons General. Human frailty must be given due consideration in determining such policies. One Surgeon General may understand the military uses and requirements of each specialty: whereas another, because of honest but obstinate prejudices, may overemphasize certain specialties and neglect others. Clearly the only way to balance out this uncertain human variable is by giving every relevent specialty a clearly recognized place

in the organization of military medicine.

In the last war, and again in the present struggle, the rapid mobilization of medical manpower demonstrated that if there is not full recognition of the importance of the medical specialties as necessary adjuvants to the military general practitioner, a tendency arises to waste highly trained personnel, and many avoidable misunderstandings and delays occur. Thus it took two years from the date of passage of the Selective Service Act of 1939 until a chief of a neuropsychiatric branch was appointed in the Office of the Surgeon General of the Army. Had such an officer been continuously at work over the years which have intervened since the last war, we would not have had to wait three years for the organization of psychiatric dispensaries to parallel the medical and surgical dispensaries in army camps, and four years for the recognition of the need for medically and psychiatrically trained social workers to function as clinical investigators.

Comparable problems have arisen with respect to plastic surgery. In this highly specialized field there are about 150 certified diplomates in this country, with almost no representation in the armed services prior to the emergency. Yet both in peace and in war the repair of mutilating injuries creates an incessant demand for the highly trained and experienced specialist. In civilian practice he is called upon by the orthopedic surgeon, by the neurosurgeon, by the pediatrician, and by all who deal with burns and industrial accidents. In peace as in war the armed services need him as a teacher, as a surgeon in difficult cases, and as a consultant in many others. He is needed also as a research worker to reproduce in the laboratory the mutilations caused by the changing weapons of war, and to experiment with the plastic restoration of destroyed tissues. Certainly it is inconceivable that we should offer surgical care to the soldier that is less skilled than the best which is available to the industrial worker.

In the absence of the true specialist in plastic surgery among the regular medical officers, one finds that there is inadequate appreciation of the need for plastic surgeons, and wholly inadequate plans for their use, their mobilization, and their emergency training; whereas the presence of such specialists will make it possible to assemble, organize, and train younger men during wartime emergencies. Through membership in national specialist societies the military regular who is a specialist in plastic surgery will be in touch with young plastic surgeons who are in

training all over the country, and with promising young general surgeons who under the stimulus of war can be developed into plastic surgeons for the needs of war by special courses of intensive supervised operative training. Clearly here again the Surgeon General's "cabinet" should at all times include a representative of this special field.

The same conclusions will be reached from a factual and objective study of the needs of the services, in peace and in war, with respect to almost all other medical and surgical specialties. The Offices of the Surgeons General should be composed of specialists, like the faculty of a medical school or the medical board of a teaching hospital.

The fact that a medical officer on an isolated post or ship must be prepared to meet all medical emergencies as best he may, is no more incompatible with the co-existence of the military specialist than is the co-existence of the country doctor and the specialist in civilian practice. As a matter of fact the armed services have approximated this very plan in the fields of tropical medicine and of sanitation. Our argument is essentially that similar considerations are applicable to all other specialties.

Innumerable examples could be given of the long delays which the existing system has caused in the appropriate use of medical men of all ranks as they were drawn from civilian medicine into the military forces. These delays cannot be explained away as due to a wise and wholesome policy of broad initiation into military life, because they have lasted long beyond any reasonable period of indoctrination. Nor should such delays be viewed indulgently as an unavoidable result of the speed with which the military organizations had to grow, because they occurred most seriously during the relatively leisurely year before Pearl Harbor.

On the other hand one can understand readily that there should be a natural tendency to continue in war the policies with which the services are already familiar in peace. It was quite inevitable therefore that at first there should be an effort to make of every incoming civilian doctor a military general practioner. As the months and years of war have passed, this tendency has lessened, but in neither the last war nor in this has the basic policy been formally abandoned. The specialist is still not sure that he will be used in his own specialty, even to the extent that his specialty is needed in the medical work of the armed forces; and the old policy is still defended long after it has practically broken

down through its sheer unworkability.

Another manifestation of this is found in some of the special service schools for the indoctrination of newly inducted civilian physicians. These usually make the error of attempting to train every medical officer for every possible task, instead of classifying the newly inducted physicians in accordance with their civilian experience and then training them with the least possible loss of time only for the specific tasks which await them. It should be taken for granted that this would not supplant the more general indoctrination of numerous military "general practitioners" for service in theaters of operation, in isolated posts, and the like. In a total war, however, in which large numbers of older specialists enter the services, a parallel system of specialized indoctrination would economize time, teaching personnel, and facilities. It is only because of the deeply ingrained tradition that every military physician must be ready to fill any post, that the pedagogically unsound and impracticable policy persists of attempting to cram the entire curriculum down every throat.

At times one wonders just how well this traditional policy works even in leisurely periods of peace. From friends in the regular services one hears disquieting things about many of the indoctrination and refresher courses which are given at the army and navy medical schools. Too often they seem to have been short and superficial, almost as though they were a formal gesture, or scaled down to the level of the physician who chose military service as a refuge, as some civilian physicians choose the state hospital service. One hears that the courses are rarely difficult, with little weeding out of the unfit through severe examinations, and with little recognition of superior ability through quick reward. Only in one or two fields do they provide an opportunity for original work. In so far as these criticisms, which have been voiced by regular medical officers, may be true (which the writer is not in a position to judge), one would seem to glimpse again the influence of a traditional reluctance to allow medical officers to become fully mature in special fields.

To these reflections one might well add a consideration of whether medical administration also should not be recognized as a military medical specialty, as is happening in civilian medicine. Major General Lull writes of "men who are listed as members of specialty societies and certificants of the various boards, who are now commanding large hos-

pitals or serving on staffs of commanding officers of task forces." This leads one to wonder whether making over a medical specialist of proved ability into an administrator of uncertain calibre is not a waste of specialized scientific knowledge which we can ill afford, particularly in times of emergency. The issue calls to mind the statement attributed to the younger Ostwald who, on his return from a visit to the scientific institutions and universities of this country said: "When the Americans discover a man who is superbly fitted for some kind of scientific work, they reward him with a position in which he can no longer pursue it." Ostwald was talking of American universities; but the challenge is equally appropriate when directed to the medical services of our armed forces, because of an apparent tendency both in peace and in war to place medical officers of outstanding scientific and professional abilities under the yoke of routine administrative responsibilities.

One can at least picture a way of using the specialist in the armed services which might be scientifically sounder and at the same time more practical than existing policies. The military physician knows the ways and needs of the army or navy. A military physician who was at the same time a specialist would know both the needs of the services and the scientific requirements of his specialty as well. Furthermore, through his membership in national specialty societies, through his attendance at their meetings, and through his knowledge of the literature he would also have a direct acquaintance with the personnel available in that field. It is clear that such knowledge as this could be used to expedite greatly the induction and the proper utilization of specialists during any period of rapid mobilization and expansion.

The arguments against intensive specialization in the armed services assembled by Captain Carpenter (loc. cit.) do not seem to be unanswerable. For instance, he quotes Admiral Rossiter as saying that he has never had difficulty in placing naval medical specialists in positions where they could use their specialties. Again, it would seem to be not impossible to reserve such training for those who had already achieved a broad general training. Nor would it seem difficult to avoid the danger of losing the specialists to "the more lucrative fields of civil life", if such training were offered only on the express condition that the officer undertakes to remain in the service either permanently or else for a specified minimum number of years. In short Captain Carpenter's points seem to be important considerations for which plans must be

carefully made, rather than insuperable obstacles which make it impossible for the armed services to benefit by the presence of fully matured specialists in their own ranks.

These are some of the many reasons why it seems appropriate to ask whether it is not necessary to supplement the highly trained corps of general practitioners of what Admiral McIntire so aptly calls the "specialty of military medicine" with a large number of medical specialists, in the civilian sense of the term. A further question may be raised whether this may not best be achieved by further developing the Army and Navy medical schools to serve as centers of research and of postgraduate instruction in all special fields, with exchange professorships with civilian medical schools, and with adequate appropriations for laboratories, research, and publication.

For as we have said, mastery of a specialty is inadequately indicated by membership in specialty societies, or by special diplomas or certificates. Such mastery is best indicated by the teaching which the physician does in that field, and by the original contributions which he makes to it. It takes time to teach, to make original contributions, and to write. If we are to have fully competent specialists in the medical ranks of the regular armed forces, the life of the regular medical officers must allow time which can be devoted to this kind of work. Otherwise, able men may never have an opportunity fully to utilize their abilities in their fields of special interest and training; and in the end this will be reflected in the calibre of the physicians who serve in the regular forces of our Army and Navy.

Summary

Since the last World War, large general hospitals have been developed as part of the medical installations of the Army and Navy. Such hospitals require the use of specialists in increasing numbers. Consequently a tacit, partial and unofficial recognition has been given to the need for specialists in the medical services of the armed forces. Qualifications for military specialization have not been clearly indicated, nor correlated with the rigorous principles and standards of National Specialty Boards. Nor have the many problems attendant upon this development been worked out fully. It would seem to be essential, therefore, that as an outcome of this inevitable trend and of the experiences of the last war and of this one, certain careful objective investigations should

be undertaken as soon as possible by a joint board, to consist of regular medical officers from the services, of civilian specialists who have served in the armed forces during the war, and of representative leaders in medical education, hospital organization and medical-social administration from civilian life. Such an inquiry might well be launched before the end of the war, as its end is clearly approaching.

In a peaceful democracy the impetus towards studies such as these becomes strong only during a period of wartime crisis. The investigations themselves cannot be concluded in such a period; but the need for them can be pointed out and they can be initiated, so that when the war is over the lessons learned during war will not be forgotten by the civilian medical profession as a whole, by national specialty societies, by national qualifying Boards, nor by the military themselves.

THE IMPORTANCE OF THE Rh FACTOR IN MENTAL DEFICIENCY

*A Preliminary Report**

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APPROXIMATELY 30 per cent of admissions to institutions for mental defectives cannot be classified etiologically on the basis of our present knowledge. This, so-called, undifferentiated group represents a distinct challenge for medical investigation. The recent discovery of the Rh factor[1] and the demonstration of its importance in producing a characteristic type of fetal injury, namely, erythroblastosis fetalis[2] may be of importance in this respect. The possible relationship of Rh iso-immunization and certain types of mental deficiency is best described by the following brief review.

Some years ago we reported the pathological changes in the brain of infants who had died as a result of icterus gravis.[3] This pathological condition, known as Kernicterus, included widespread ganglion cell injury in the cerebral cortex, cerebellum, basal ganglia as well as other structures. Later, we reported the clinical picture of children who had recovered from icterus gravis but subsequently exhibited evidence of central nervous system injury. This included severe mental deficiency, extra-pyramidal spasticity and athetosis. An autopsy on one of these children confirmed the relationship of the cerebral changes originally described as Kernicterus and the above mentioned clinical picture.[4] At that time the etiology of icterus gravis was unknown. Beginning with the clinical studies of Diamond, Blackfan and Baty,[5] it was demonstrated that icterus gravis represented one manifestation of the syndrome erythroblastosis fetalis. In 1941, Levine and his co-workers[2, 6, 7] clearly indicated the importance of Rh iso-immunization in the etiology of erythroblastosis fetalis. Since Kernicterus is found primarily in children with erythroblastosis, it would appear reasonably well established

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that the pathogenesis of the cerebral changes in this condition was in some way related to the Rh factor. At present, therefore, one may justifiably implicate Rh iso-immunization as the etiological mechanism in individuals with the following characteristics: (1) mother, Rh negative; (2) patient, Rh positive; (3) neonatal history of erythroblastosis fetalis; (4) evidence of basal ganglion disease; (5) severe mental deficiency. However, if this syndrome were the only manifestation of Rh iso-immunization, it would play a very insignificant role in the over-all picture of mental deficiency. Thus, at the Southbury Training School for Mental Defectives only one child was admitted with all these characteristics, among 1200 total admissions. There are, however, certain observations that suggest that Rh iso-immunization may play an etiological role in cases where basal ganglion disease is not manifested, and where a history of neonatal erythroblastosis is not obtained. These observations are of two types. In certain of the autopsied cases showing Kernicterus, the clinical picture was not characteristic of erythroblastosis fetalis, in that the blood examination was not considered abnormal, and the jaundice was minimal. Also, we have recently encountered patients with typical familial and clinical histories of erythroblastosis fetalis whose neurological disorder was essentially that of cerebellar dysfunction rather than basal ganglion defect. Similar discrepancies have been recorded in the literature.[3, 4] On the basis of these considerations there exists the possibility that certain of the imbecile and idiot defectives, now so unsatisfactorily classified as undifferentiated, may be etiologically explained as probable results of Rh iso-immunization.

Fortunately, this hypothesis lends itself to investigative confirmation. In a random sampling one would expect to find from 13 to 15 per cent of individuals, Rh negative. If a significantly greater proportion of mothers of an unselected group of undifferentiated defectives were found to be Rh negative, it might reasonably be deduced that in at least a number of these defectives, the mechanism of Rh iso-immunization was of etiological importance. In a preliminary survey, the incidence of Rh negative mothers of an unselected group of undifferentiated mental defectives was found to be approximately 25 per cent. On the other hand, among an equal number of mothers of mongolians, diplegics, microcephalics, etc., the incidence of Rh negative blood was in the normally expected range of about 12 per cent. While the total

number examined to date (approximately 100) is too small to draw definite conclusions, the difference between the two groups is statistically significant. The results are of sufficient interest to warrant further study.

While the demonstration of an Rh negative mother of an Rh positive mental defective does not make the diagnosis of fetal central nervous system injury due to maternal Rh iso-immunization, it does indicate the group in which this is a distinct possibility. Further clinical study of a large group of this type, may eventually result in a clinical characterization for which the blood studies may have confirmatory value.

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